aim of science. It is impossible to estimate the good that might be done in this direction, if only the Universities had the wisdom and the courage to insist upon a knowledge of some one branch of science for all degrees, as was strenuously advocated years ago by Charles Kingsley.

(3) It seems a great pity that such a change as is contemplated should be adopted just now, since within the last three or four years some of us who are teachers of standing and experience have gladly recognized considerable improvement in the examinations in science as they are conducted by the Civil Service Commissioners. It would appear that the *cramming* of these subjects has been considerably handicapped, if one may judge from the considerable increase in the number of o's affixed to the names of candidates in the published lists *pari passu* with a considerable increase in the number of marks gained by one's own pupils, who have had the same teaching and laboratory training as those of previous years.

(4) It is surely fairly within the province of Parliament to consider the question whether it is expedient or conducive to the common weal, that science shall be placed at such a disadvantage that young men, who are candidates for the more scientific branches of the military service, shall be strongly tempted to eschew all preliminary training in science, as they certainly will be unless the regulations are somewhat modified.

Four years ago the action of a single Member of Parliament (Sir John Lubbock), backed up by the influence of the Councils of the Royal Society and the British Association, was effectual in securing a reconsideration of the provisional examination scheme for admission to Sandhurst; so that, although—as ultimately issued—the regulations contained an absurd anachronism in the proportion of marks assigned to scientific subjects, this was reduced to less startling proportions.

Can it be doubted, then, that if on the present occasion the three Members of Parliament who may be said to be the representatives *par excellence* of science in the Legislature (the President of the Royal Society, the President of the British Association, and the Member for the University of London), were to take united action in Parliament, the position of science (so far as the Army Entrance Examinations are concerned) might be changed from one of semi-strangulation to one of free and fair competition, which is all that its most ardent advocates can desire for it? This could be effected to the advantage of the studies of the cadet, by such a simple modification of the published regulations as would be involved in limiting the choice of optional subjects in Class I. to two, and allowing two subjects of Class II. to be taken up. A. IRVING.

Wellington College, Berks, February 20.

The Composition of Water.

PROF. THORPE, in his interesting article on the composition of water (p. 313), alludes to Dr. Scott's very valuable determinations of the ratio of the volumes of hydrogen and oxygen which combine to form water.

If we assume with Dr. Scott that the small amount of impurity present in his gases, and which he estimated in each case after the explosion, was evenly distributed between them, a curious relation may be observed between the amount of impurity present and the calculated ratio of the volumes.

This relation is apparent if we subtract the ratios calculated by Dr. Scott from some fixed number, say 2·000, and compare these differences with the relative amounts of impurity. It will be seen from the following table that the greater the amount of impurity present the greater is the difference of the ratio from the constant number, or, in other words, the lower is the ratio. The impurity is given in volumes per 100,000, and the differences are multiplied by 10,000.

Relative impurity.	Difference of ratio from constant.	Relative impurity.	Difference of ratio from constant.
35	83	102	125
36	55	105	133
41	66	106	140
47	44	146	154
52	32	162	106
56	90	167	224
66	72	188	118
72	46	254	187
75	80	495	540
78	120	498	506
98	73		

The relation is better seen, however, by mapping the results, taking the ratios as abscissæ, and the impurity in volumes per 100,000 as ordinates. The dotted straight line (Fig. 1) passes so well through the points that it leaves ten of them on the one side and eleven on the other.

It seems difficult to believe that this apparent relation can be merely a chance coincidence; the direction taken by the points is too definite. Nor can it well be due to any chemical action between oxygen and nitrogen, for Dr. Scott states that the water produced was free from any acid reaction, and that no trace of the oxides of nitrogen could be detected. The relation is even more marked if we assume that the whole of the impurity was in the hydrogen. This is shown in Fig. 2; the points obviously fall about a line which is nearly, if not quite, straight.

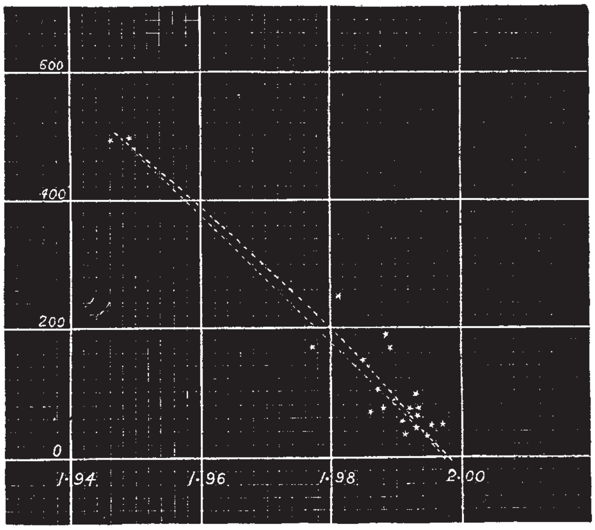

FIG. 1.

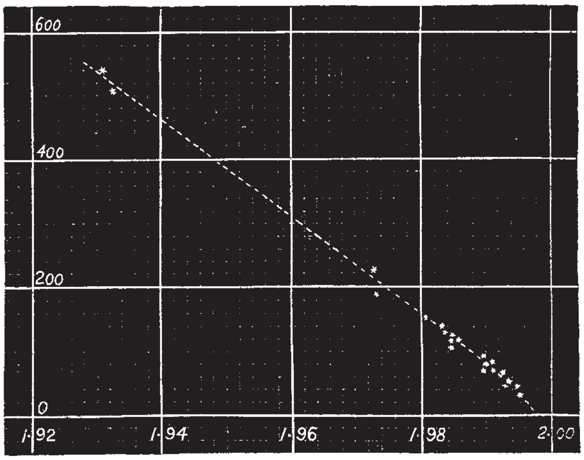

FIG. 2.

If, however, we assume that the whole of the impurity was in the oxygen, and if we neglect the two experiments with the excessive amount of impurity, no such relation is to be observed (see Fig. 3), but the ratios are distributed with fair regularity about a mean value of 1·9965 or 1·9970. The simplest explanation of the facts appears to be that the whole, or at least the greater part, of the impurity was really in the oxygen, and that the apparent relation of the amount of impurity to the ratio

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is due to the error introduced into the calculations by referring the impurity to the hydrogen. But, whether this explanation be accepted or not, it is clear that the three lines drawn through the points representing the three series of ratios ought to meet at a point on the horizontal line of zero impurity, for the errors, whether due to chemical action or to calculation, would disappear with the cause that produced them. Hence, if no other source of error is present, the true ratio may be found by taking the most probable point of intersection of the three lines on the horizontal line of zero impurity. It is not easy to determine exactly the position of this point: it probably lies between the values 1·996 and 1·998, and the true ratio may perhaps be taken as 1·997 Dr. Scott adopts the ratio 1·994, but this appears to me to be certainly a little too low.

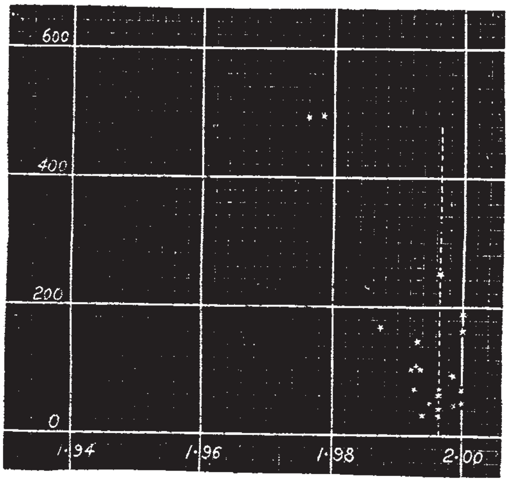


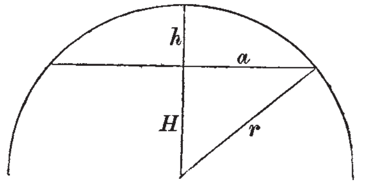
FIG. 3.

Prof. Thorpe shows that the atomic weight of oxygen, calculated from Regnault's densities of oxygen and hydrogen, corrected by Prof. Le Conte, and Dr. Scott's ratio (1·994) for the combining volumes, is 16·009. The ratio 1·997 would make the atomic weight O = 15 985. SYDNEY YOUNG.

University College, Bristol.

The Fog Bow and Ulloa's Ring.

IN the summer of 1875, I made a tour of inspection to our meteorological stations in the surveying-steamer *Hansteen*, Capt. M. Petersen, R.N. During the morning hours of August 7, I was on shore at Gandfjord, on the south side of the Varangerfjord, and measured the height of some terraces there. At 1h. 10m. p.m. we took serial temperatures in the Gandfjord with the deep-sea thermometer. The weather was calm, and a dense fog prevailed. The temperature of the air was 12° C. Leaving the Gandfjord we proceeded northwards. The dense fog continued. At once the fog began to be lighter and the sun to shine through, and a few minutes afterwards we were out of the fog, which was standing as a white wall in the south-west. In the moment the sun appeared, but before we were quite clear of the fog, I saw in the north-east a bow having the shape of a rainbow, but quite white, projected on the fog. With a sextant I measured its amplitude, or the chord along the horizon, and the height of the summit above the horizon—in both cases the middle between the outer and inner edge of the bow. The horizon not being distinctly visible, it is probable that the measures taken do not exactly refer to the true horizon, nor is it certain that the height of the summit was taken from the same horizontal plane in which the amplitude was measured. By the captain's reckoning, the apparent ship's time, at the moment of observation, was 2h. 40m., and the latitude 70° 1′. From these data, and the declination of the sun, I computed the azimuth of the sun at south 46° 5′ west, and its apparent altitude at 31° 12′. Supposing, as the results of the several computations tend to indicate, that the white bow is circular, and has its centre in the anthelic point, we may calculate the angular radius of the bow by three different methods.



Let h represent the height of the summit of the bow above the horizon, a half the amplitude or chord along the horizon, H the dip of the centre of the bow below the horizon, supposed to equal the altitude of the sun, and r the angular radius of the bow. Then we have—

$$r = \mathrm{H} + h \quad \ldots\ldots\ldots\ldots \quad (1)$$

$$dr = d\mathrm{H} + dh \quad \ldots\ldots\ldots\ldots \quad (1')$$

$$\cos r = \cos a \cos \mathrm{H} \quad \ldots\ldots\ldots\ldots \quad (2)$$

$$dr = \frac{\tan a}{\tan r}\, da + \frac{\tan \mathrm{H}}{\tan r}\, d\mathrm{H} \quad \ldots\ldots \quad (2')$$

$$\cos r = \cos a \cos (r - h)$$

$$\tan r = \frac{1 - \cos a \cos h}{\cos a \sin h}, \text{ or putting}$$

$$\cos a \cos h = \sin^2 \mathrm{M}$$

$$\tan r = \cot^2 \mathrm{M} \cot h \quad \ldots\ldots\ldots \quad (3)$$

$$dr = \cos (r - h) \frac{\cos r}{\sin h} \tan a\, da - \sin (r - h) \frac{\cos r}{\sin h}\, dh \quad (3')$$

The observations gave $2a = 49°$, $a = 24° 30'$, and $h = 7°$ (or a little more).

From (1) we have $r = 31° 12' + 7° = 38° 12'$.

Putting $d\mathrm{H} = \pm 2'$, $dh = \pm 15'$, we have by (1′)

$$dr = \pm \sqrt{2^2 + 15^2} = \pm 15'{\cdot}1 = \pm 0°{\cdot}25.$$

From (2) we have $r = 38° 53'{\cdot}5$,

and by (2′) $dr = 0{\cdot}577\, da + 0{\cdot}770\, d\mathrm{H}$, $d\mathrm{H}$ being the error in the altitude of the measured chord, or the chord's altitude or depression, reckoned from the horizon.

Putting $da = d\mathrm{H} = \pm 15' = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}24.$$

From (3) we have $r = 41° 8'$,

and by (3′) $dr = 2{\cdot}315\, da - 3{\cdot}074\, dh$.

Putting $da = dh = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}97.$$

Taking the weights inversely as the squares of the probable errors, we find that the results from (1) and (2) have a weight of 15 times that found by (3), and the mean will be—

$$r = 38° 38' \pm 6'{\cdot}4.$$

From this mean we find that h should have been 38° 38′ − 31° 12′, or 7° 26′ instead of 7°, or somewhat greater than measured, as supposed in my note-book. Computing from (2) we find that we should have calculated with H = 30° 51′ instead of 31° 12′, or the chord has been measured in a level 21′ lower than the horizon, which is highly probable with the fog spreading over the surface of the sea. The measured chord being too great, and the measured height too small, it follows from (3′) that (3) must give the radius by far too large.

The next occasion I had to observe the fog bow was in 1878, on the North Atlantic Expedition, when returning from Spitzbergen. During August 30, our ship, the *Vöringen*, had a rather tedious work in advancing southwards, on account of the foggy weather prevailing the whole day. In the afternoon we had advanced so far south of Bodö as to approach the Sandhorn, a mountain about 3000 feet high, lying to the east of the route.

At 5h. 20m. p.m. I saw an anthelic fog bow, white, with the outer edge reddish, the inner edge bluish. I measured, with the sextant, the amplitude along the horizon at 76°, the sun's altitude at 12°, and the breadth of the bow at 2°. The temperature of the air was about 14° C. The latitude was about 67° 10′.

Assuming the measured chord to lie in the true horizon, we get by (2) from $a = 38°$, $\mathrm{H} = 12°$, $r = 39° 35'{\cdot}5$. But it is highly probable, that the measured chord lay deeper than the

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